

A Hydroclimatic Database for Distributed Hydrologic Modeling in CA

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0316: A Hydroclimatic Database for Distributed Hydrologic Modeling in CA

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The investigators propose to develop a hydroclimatic data base for use by others interested in conducting distributed hydrologic modeling in California. The model would use existing atmospheric data over the period 1960-2002 and provide a grid-size resolution of 9 km. Although the goals of the study are clear, justification is lacking and the approach is suspect. Primary criticisms are: 1) the underlying atmospheric data used in the model are at a coarser scale than the proposed grid-size output, resulting in uncertain interpolation to scales not supported by the data; 2) model validation is not discussed; 3) relevance of model selection, grid size, and time series (1960-2002) is unspecified; 4) users and need for model are not well defined, consequently value is uncertain; and 5) two of the reviewers indicate that the idea is not novel, and that relevance to similar projects underway at other institutions should be discussed/recognized.

Additional Comments:

The investigators propose to develop a hydroclimatic data base for use by others interested in conducting distributed hydrologic modeling in California. The model would use existing atmospheric data over the period 1960-2002 and

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Technical Synthesis Panel Review

provide a grid-size resolution of 9 km. Although the goals of the study are clear, justification is lacking and the approach is suspect. Primary criticisms are: 1) the underlying atmospheric data used in the model are at a coarser scale than the proposed grid-size output, resulting in uncertain interpolation to scales not supported by the data; 2) model validation is not discussed; 3) relevance of model selection, grid size, and time series (1960-2002) is unspecified; 4) users and need for model are not well defined, consequently value is uncertain; and 5) two of the reviewers indicate that the idea is not novel, and that relevance to similar projects underway at other institutions should be discussed/recognized.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Two of the three external technical reviewers and the panel concluded that the proposed work was technically deficient. The third external technical reviewer gave this proposal a more favorable rating. However, the review did not provide any detail substantiating the rating. It was not clear to the panel how this project could provide new information. In particular, the North American Regional Reanalysis (NARR) project has produced a 20-year high resolution time-series of surface climate at 32-km resolution. The NARR project is far superior to the proposed project for several reasons: 1) the project is completed and data are available; 2) the NARR assimilates observations within the model domain, whereas the model runs proposed by the applicants are only forced at the lateral boundaries; and 3) the NARR has a clear plan for distributing the large volume of data produced. This last point is especially relevant, as the applicants include no plan for data distribution. It is not clear if the (non-assimilating) 9-km run proposed by the applicant will be superior to the NARR output. Further, the applicants have only published short articles in engineering journals and have not previously undertaken extensive analyses of their model output.

Technical Review #1

proposal title: A Hydroclimatic Database for Distributed Hydrologic Modeling in CA

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	Yes, the goals, objectives and hypotheses are clearly stated. The proposed work is important as the data might be used in the hydrological, ecological model with higher spatial and temporal resolution, however this kind of idea is underway elsewhere (i.e. Lawrence Livermore National Laboratory).
Rating	good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The study is justified relative to existing knowledge. The proposal clearly outlines the concept and methodology and explains the underlying basis. The selection of research is justified.
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to

Technical Review #1

generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approach is appropriate and feasible for achieving the objectives of the project. However, the approach (methodology) may overlap with other underway projects for the same domain in CA. Since the project simulation is for 42 years (1960–2002), the results might be useful other hydrological, ecological and environmental project to investigate the historical nature of systems. The project outcome has indirect relation to decision makers.
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The approach is well documented and technically feasible. The project might be a successful, as the team has strong research background and have performed research for different project with similar approach.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The approach does not outlined any monitoring design, however the results for 9km grid and hourly time resolution should be checked with the observed data to investigate the model performance.
Rating	good

Technical Review #1

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The results from the project may be used in various models, and has scientific values. The project outcome will likely contribute to the larger data management system.
Rating	very good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The project team has nice track record and is capable of performing this project. The University of California-Davis is a well-reputed institution for research and has infrastructure to accomplish this project.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The proposed budget is reasonable and adequate.
Rating	very good

Technical Review #1

Overall

Provide a brief explanation of your summary rating.

Comments	The project develops database for higher spatial and temporal resolution data that might be useful for hydrological, ecological and other models. If the project is not overlapping with other project, the results may add data to database.
Rating	good

Technical Review #2

proposal title: A Hydroclimatic Database for Distributed Hydrologic Modeling in CA

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goal is to re-construct rainfall and other atmospheric data over California at 9-by-9 Km and hourly spatio-temporal resolution, for the 1960-2002 period. The idea is good but not entirely novel: The reviewer is aware of other efforts at similar or somewhat coarser resolution.
Rating	good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The work is not quite justified because the coarser data set (NCAR/NCEP Reanalysis) that will be providing initial/boundary conditions for the finer resolution MM5 model is, itself, unable to realistically represent hydroclimate variability on regional-to-subcontinental scales. The large scale context for the proposed high resolution analyses will thus be flawed, severely limiting the value of the planned exercise.
Rating	

Technical Review #2

	fair
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Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approach is reasonable, but sort of 'brute force' in nature. It lacks innovative elements. Moreover, little is said about how the new product will be evaluated. A high-resolution physical data set will indeed be produced, but how will we know that it is good, and decidedly better than a sophisticated interpolation of the coarser, controlling fields. Why will the quality and integrity of atmosphere-land-surface interactions be better here than in other regional reanalysis products, e.g., the North American Regional Reanalysis and the one being produced at Scripps. Is there a pilot study that demonstrates the superiority of this approach?
Rating	fair

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The approach is technically feasible
Rating	good

Technical Review #2

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Not Applicable
Rating	good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The project may generate products of interest; a pilot-phase demonstration is needed, though. The project will however be deemed successful only if it generates derived products for hydroclimate applications and ecosystem monitoring; and not just a physical data set, as seems likely at the present.
Rating	good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The PI is technically competent in producing MM5 data sets but, perhaps, not in evaluating them. High-resolution will, in itself, not be enough. To be sure, MM5 adds some value to the analyses, but how much? The proposal doesn't address this issue at all. The investigative team's expertise in evaluating the integrity of hydroclimate data sets appears to be
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Technical Review #2

	somewhat limited.
Rating	good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Excessive
Rating	fair

Overall

Provide a brief explanation of your summary rating.

Comments	Producing high-resolution observational analyses is not intellectually challenging, but generating those with improved representation of land-atmosphere interaction is. Without these improvements, the new data sets will be of very limited value. The proposers have ignored the issue of quality. A pilot study demonstrating quality improvement should be a pre-requisite.
Rating	fair

Technical Review #3

proposal title: A Hydroclimatic Database for Distributed Hydrologic Modeling in CA

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	No explanation is given as to why this particular time span is important for the region. It is also not clear as to why the spatial scale 9 km X 9 km grid size is suitable for this region. What are the scales of geographical features that have a significant impact on the hydrology of this region? If the purpose of this project is to calibrate hydrological model, using precipitation from a fixed time period will generate "noisy model tuning" parameters that can not be used for forecasting future run-off. Who are the stakeholders that need to have this data?
Rating	poor

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	There is no reference to studies on "Intercomparison of global reanalyses and regional simulations" carried out by different research groups, e.g., U of Washington. Knowledge of the results from this type of research is crucial prior to using NCEP precipitation data as an input to MM5. It is also not clear as to why the spatial scale 9 km X 9 km grid size is suitable for this region. What are the scales of
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Technical Review #3

	geographical features that have a significant impact on the hydrology of this region? It is not clear how methodology created for this study area (for 1962–2002) can be extended to carry out longer (climatologically relevant) time priods.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	Why mentioning RAWINS program if it will not be used? If the parameterization used in MM5 will include MRF (what's that stands for?) boundary layer scheme, where would data about the boundary layer come from? NCEP reanalysis generates many parameters will all of the be used? How would authors carry out quality control of the dataset created? Why this type of information will be useful to decision makers?
Rating	poor

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The goals (not justified) stated in this proposal do not math the list of tasks descrbed in the tasks section. The reasons for selecting models and methodology are not supported by references. It is not clear why selected grid is appropriate for the study area. What features/capabilities of MM5 are relevant for this region? The description of methodology used is limited to generating 9km grid input, there is no discussion on how the quality of the database will be
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Technical Review #3

	evaluated. The objectives stated are within the grasps of authors but the (scientific and/or technical) usefulness of this project is not fully supported by the text of this proposal.
Rating	fair

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	There is no description in this proposal of using statistical (or any other type of) analysis tools that would reassure future users about the quality of the datasets. It is not clear what type information will be in the reports. Judging by the budget assigned (14%) to this task, the authors do not consider project progress monitoring and project outcome analysis to be very important.
Rating	poor

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	There is nothing in the proposal to indicate that the analysis of user's (whoever they are) needs and requirements was (or will be) carried out. The "Fields of Dream" approach may work in the films (money generating venture) about baseball but does not work very well with scientific knowledge. Products (DVDs with data) will be generated in this project. There is no information within the proposal as to the quality control, interpretation of the results, data management tools required for
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Technical Review #3

	these datasets, and/or how (and why) future users should use these data.
Rating	poor

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The project team members have very impressive backgrounds. Too bad that they did not put more effort into writing this proposal. There is sufficient infrastructure support to carry out this type of project, provided project goals reflect more the team capabilities.
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget breakdown does not match time allotments listed in the Tasks section. According to that section, task 1 is of a 12 month duration. The budget section assigns to that task ~36% of the budget over 3 year period. What technical challenges are expected that justify the amount dedicated to this task? The modeling efforts is assigned 49.4 %. There is no explanation within the proposal why this task requires largest budgetary commitment, whereas, project management, post processing (do the authors imply: quality control and/or analysis of the results) is only worth 14% of the
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Technical Review #3

	budgetary effort.
Rating	poor

Overall

Provide a brief explanation of your summary rating.

Comments	The information provided in this proposal is not adequate to justify requested funding amount. There is nothing in this proposal to indicate that this project will add to the current state of the scientific and technical knowledge or provide tools to further understanding of the links between past climatology of precipitation and possible critical conditions that may occur in the future.
Rating	poor

